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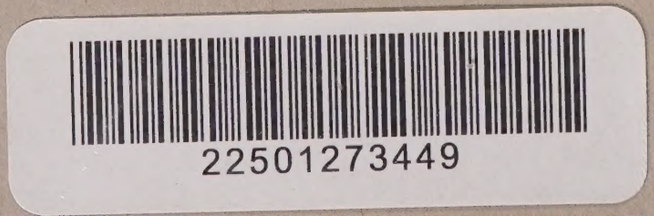

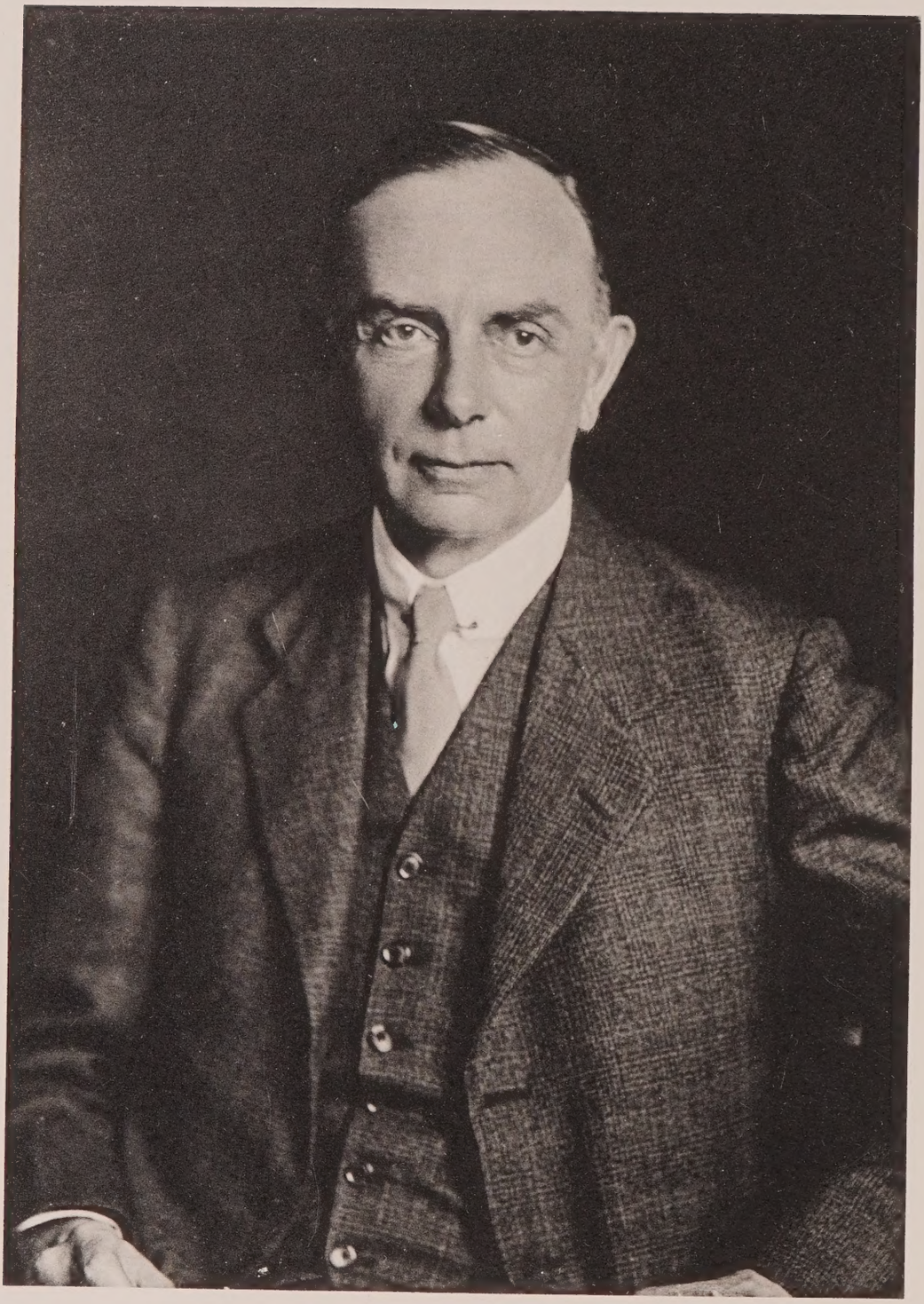

R. P. Landlaw

[From *Obituary Notices of the Royal Society* 1940]

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PATRICK PLAYFAIR LAIDLAW

1881—1940

Sir Patrick Playfair Laidlaw—'P.P.' to a circle of colleagues and contemporaries who enjoyed his friendship and held him in affection—died suddenly of heart failure in the night of 19–20 March 1940. He was born on 26 September 1881, in Glasgow, being the third son in a family of six sons and one daughter. His father, Robert Laidlaw, M.D., F.R.C.S.(Edinb.), was at that time Superintendent of the Glasgow Medical Mission, later held a similar post in London, and still later, from 1901–1908, served as Magistrate and Medical Officer to the Seychelles Islands. His mother, born Elizabeth Playfair, came of a family which has produced several men of eminence in learning, science and medicine. Her father and grandfather were both named Patrick Playfair, and the latter married a distant cousin, the daughter of Principal Playfair of St Andrews University. Two of Principal Playfair's grandsons, sons of Surgeon-General Playfair who did distinguished medical service in India, and first cousins of P. P. Laidlaw's maternal grandfather, were well-known figures in science and medicine in Victorian times. One of these was Sir Lyon Playfair, F.R.S., later the first Baron Playfair, who, having held several important Chairs of Chemistry, including that at the Royal Institution, and taken a prominent part in the organization of the Great Exhibition of 1851, entered Parliament, became Deputy-Speaker of the House of Commons and later Postmaster-General; after his elevation to the peerage he was a Lord-in-Waiting to Queen Victoria. The other was Dr William Playfair, who took high rank among the physicians of his day as a specialist in obstetrics and gynaecology.

In 1887 Dr Robert Laidlaw accepted appointment to a Medical

Mission in the North of London, moved there with his family, and stayed for the next ten years. Before this removal from Glasgow, at the age of three years, P.P. had suffered from an attack of infantile paralysis, which left him with a permanent weakness of one leg and some deformity of the foot. While the family lived in North London he attended a private Quaker School named 'Paradise House', but there is no record of any special achievements there. The atmosphere of his home in those days, more typical, perhaps, of his parents' native Scotland than of the London of their adoption, was one of quiet culture, evangelical piety, and slender means in relation to the needs of a large family. Nor was physical contact with Scotland allowed to lapse, for the devoted parents, with a heroism which I often heard Laidlaw recall with gentle humour, contrived regularly to transport their large and growing family for a month in each summer from London to the Ayrshire coast. So, early in life, Laidlaw acquired a feeling, which he never lost, for Scotland as the only real holiday country for one who knew its charms. On these early holidays he learned to fish and to play golf, and from both he got pleasure and health until, after middle life, increasing lameness restricted his activity.

In 1897 Dr Robert Laidlaw removed to Cambridge, in the interests of the further education of his family. Two of the boys entered the Leys School as day boarders, P.P. being the younger of these; and there, in 1899, I first came across him, a shy and rather delicate lad of sixteen, as a member of a Biology class which I was teaching in spare time, during my fourth year at the University. In a small class of rather more than the average ability, most of them having a medical course in prospect, he was outstanding in the quickness of his apprehension, the eagerness of his interest and his desire to penetrate deeper than the descriptive level of the class textbook. I saw more of him during the school holidays when, his home being in Cambridge, we spent some days together in the school museum, rearranging, mounting and labelling some collections of zoological specimens which had fallen into disorder. Under such conditions it was

possible to get past mere shyness, and to recognize that he had less natural facility of expression, whether with tongue or pen, than many boys with much less able and active minds. To some extent, I think, this difficulty remained with him through life. He was certainly not lacking in literary instinct. He had, indeed, a keen appreciation and a very retentive and accurate memory for passages of prose or poetry which had touched his imagination; but the description even of his own observations and the expression of his own ideas always cost him effort, and he shrank from speech in public, even in informal discussion of a subject in which he was a master.

Laidlaw left school in 1900 and entered St John's College, Cambridge, with a scholarship. I lost sight of him then for some years, having gone to London to complete my own medical studies. In 1904 I met him again, and was struck by his development in physique and personality, and by the remarkable promise in research which he was even then showing, although the Natural Sciences Tripos (Part II) was still before him. He had given more than an ordinary student's interest to descriptive human anatomy, having already published, at the prompting of the late Professor A. Macalister, F.R.S., several notes on anomalies and variations which he had encountered in the dissecting room, or studied in the anatomical museum. A little later he had fallen under the spell of the teaching and inspiration of Dr (now Sir Frederick) Gowland Hopkins, whose department was then a relatively new centre of interest in the Cambridge School of Physiology. The young Laidlaw, on his own initiative as Hopkins told me at the time, had made a series of highly original and important observations on the derivatives of haemoglobin. He had shown that iron is removed much more readily from reduced than from oxidized haematin, so that haematoporphyrin is obtained by relatively mild treatment from reduced haemoglobin, through reduced haematin. This observation gave strong support to Hoppe-Seyler's supposition that the oxygen of oxyhaemoglobin is combined directly with the iron. Interpreting these results as indicating further that the haematoporphyrin grouping

is preformed in the haemoglobin molecule, and not produced by intramolecular rearrangement, Laidlaw had proceeded to show that reduced alkaline haematin could, in fact, be resynthesized by treating haematoporphyrin with a ferrous iron salt (in Stokes's fluid) in the presence of a reducing agent, and had fully identified his artificial haematin with the natural pigment. Emboldened by this success he had proceeded to make and to isolate an artificial pigment practically identical with 'turacin'—a copper-containing pigment from the feathers of a family of birds—by simply boiling haematoporphyrin with an ammoniacal copper solution, and had further given a preliminary description of a compound of haematoporphyrin with cobalt. In relation to the very imperfect knowledge available at that date, these observations would have represented a noteworthy contribution, even from an investigator of mature experience. As the individual achievement of a young man of twenty-three years, not yet through his student course, they gave promise of very unusual powers. The short paper of some eight pages in which he eventually published them was a minor classic, rich in accurate observations made in logical sequence, presented clearly and with modest simplicity.

From Cambridge Laidlaw went with a scholarship to Guy's Hospital to complete his medical course, entering upon his studies there in October 1904. There can be no doubt that he continued there to show distinguished promise in more than one direction, a Gold Medal in Medicine being among the awards which he won. When I made touch with him again, in 1909, there was something like competition among his teachers and seniors to interest him in different choices of a career, and he was embarrassed by the need for a decision between clinical medicine, pathology and physiology. In the last of these he was already acting as demonstrator in the Guy's Medical School. Chance, with the need to earn a livelihood without delay, decided his course for the next four years. It happened that he and I had independently begun to study the pharmacological action of an active principle of *Apocynum cannabinum*, which we had received, each from one of two different laboratories, in which it had just been

simultaneously isolated. In neither case, as afterwards appeared, was the substance in hand the natural, unaltered glucoside, but our preliminary findings concerning its physiological action, when we met and compared them, showed that we were dealing with practically identical preparations. Laidlaw readily accepted a suggestion that we should pool our results, and finish the work together in the Wellcome Physiological Research Laboratories, Herne Hill, of which I was then the director. I had a post to offer there, for research in my own laboratory of pharmacology and physiology, and I was delighted to find Laidlaw willing to accept it. We spent the next four years in a collaboration as happy and intimate as any which it has been my privilege to enjoy. Laidlaw, having practically his whole time for research, found a number of problems to work at by himself or with others, in addition to those which he shared with me. He joined us at a time when the pharmacological properties of the amines from amino-acids, brought to light by work with Barger on ergot, had opened a number of interesting questions. Laidlaw was soon at work, with chemical co-operation from A. J. Ewins, on the fate of tyramine in the body. They were able to show that it undergoes an oxidative deamination to phenol-acetic acid, and to identify the liver as the chief site of this destructive metabolism. It may be noted that these observations provided the starting-point for work still in progress in more than one centre, with results of growing significance, on the liver deaminase and its specificity for amines of different types. At the same time Laidlaw shared with me the first detailed analysis of the extremely powerful action of histamine, which the work on ergot, again, had just revealed. The recognition of the special features of this action, and of the analogies with certain pathological syndromes which it presented, was quite as much Laidlaw's work as mine. We worked for several years on a particular paradox which the action involved, and it was to bring us together again some years later, in another connexion. Meanwhile Laidlaw had extended the work to the amine from tryptophane, indolethylamine, which we had not encountered from a natural source, and which he was the first

to produce. He found that it was readily formed by appropriate bacteria, growing in a medium enriched with tryptophane, and could be separated as a crystalline picrate. He obtained chemical co-operation again from Ewins, and together they found a method of producing indolethylamine by synthesis and studied its metabolic breakdown and excretion, while Laidlaw alone explored the details of its pharmacological action. This period from 1909–1913, when Laidlaw was working in the Wellcome Laboratories, was for him one of remarkable and varied fertility in research. The list of his publications during those years, alone and in collaboration, gives some idea of his activity and its range. He was working on biological problems which in many cases presented points of chemical interest, which had always a special attraction for him; and they offered free scope to his ingenuity and his questing curiosity. The short paper in which he showed that the direction of asymmetry with respect to a nitrogen atom may have an effect on the pharmacological potency of a substance, as great as that long familiar in the case of carbon-atom asymmetry, excited relatively little attention; but it well illustrates Laidlaw's power of seizing a point of novel interest for investigation, settling it with simple elegance and precision, recording the result with modest brevity, and then passing on again to something new. This period, indeed, though not that of the major discoveries which will be most permanently associated with his name, was that of Laidlaw's most active and varied production in research. His mind was young and eager, he was free from all responsibilities and from all but a minimum of routine duty, and, above all, he was freer than at any other period from the hampering physical disability, which in later years depressed his health and impaired his resistance to fatigue. In those good years, after a long turn of happy and concentrated research, he could go for a summer month of golf, or to his beloved Scotland for fishing and sailing, and return full of energy and zest for the work which he enjoyed. If through later life, with his experience, his judgment and his scientific opportunism fully matured, he could have kept that level of physical efficiency, and had still been able to use the

means for its renewal, he might have reached even higher peaks of distinction than those which he attained in spite of weariness and the discouragement of failing health.

In 1913 the Sir William Dunn Lectureship in Pathology at Guy's Hospital fell vacant, through A. E. Boycott's removal to the Chair at Manchester. The authorities at Guy's were anxious that the new Lecturer should be qualified to continue the development there of a school of Experimental Pathology. Laidlaw had many of the qualifications required for such a post, including a very competent knowledge of pathology and bacteriology, with an unusually wide range of interests and of technical accomplishment in other medical and allied sciences. It was not easy, however, to persuade him that it was his duty to take this wider opportunity, with its teaching obligations and numerous responsibilities. In the end he accepted, but the disaster which soon afterwards fell on the world obscured the bright prospect which seemed to be opening for Laidlaw among his old friends at Guy's, and the period of his stay there, including all the war years from 1914–1918, was one of smaller achievement in research than those which preceded and followed it. Before taking up the new appointment he had spent some months during 1913 on the continent of Europe, chiefly in Vienna and Freiburg, to acquaint himself with new developments of research and teaching in Pathology. When war broke out in August 1914 he soon found himself relatively isolated, with few colleagues remaining at their posts, and a heavy load of teaching and routine; and, though duty and disability both precluded any thought of active service for him, he was unhappy to be left when so many men of his age were going. Then two of his younger brothers, the youngest of his family, fell in action in 1915 and 1917, and their loss bore hardly on one in whose nature family affection struck very deep roots. Two years later again, in 1919, his eldest brother was drowned while crossing the bar of a Malayan river in attempting to land from a coastal steamer. Laidlaw came out of the war years sad, discouraged and in poor health, and his published record over that period is relatively slender. As always, however, it

contains items showing the ingenuity of his mind and the fertility of his imagination. Such, for example, was the short article in 1915 on 'Some simple anaerobic methods,' in which he describes the first use of hydrogen gas and spongy platinum to effect the removal of the last traces of oxygen from a culture tube and its contained medium. Later Fildes and McIntosh gave the method a wide practicability by enclosing the spongy platinum in a wire gauze jacket, on the principle of the Davy lamp. The method thus passed into general use, associated with the names of those responsible for this practically important safety device ; but it is worth while to recall that the fundamental principle was due to Laidlaw, of whose wide-ranging ingenuity it was highly characteristic. Later in the war, when shock-like conditions following severe injuries were a matter of concern and intensive study, the analogies between such conditions and the 'shock' produced by large doses of histamine brought Laidlaw into renewed collaboration with me and with Professor A. N. Richards of Philadelphia, who had come to join us in this work. After the war was over, and just before he left Guy's Hospital, Laidlaw devised and described, with W. W. Payne, a colorimetric method for estimating small quantities of calcium, as in samples of blood serum, depending on the insolubility of the compound of calcium and alizarin. We may note here again the evidence of his interest in biochemical problems and methods ; and, though in later years the centre of his attention moved away from this, his first love in science, his colleagues knew that it continued to the end to influence his thoughts and his method of approach to many problems.

Though he published few papers between 1913 and 1922, Laidlaw was continuously extending and consolidating his knowledge of pathology and bacteriology, and steadily enriching his equipment with methods of research in those fields. In 1922 the National Institute for Medical Research, which had occupied its quarters at Hampstead only two years earlier and was still in the early stages of its organization and development, was asked by the Medical Research Council, on the initiative of the

late Sir William Leishman, to formulate and put in hand a programme of researches in a field then but little explored, that of the infections due to filterable or ultramicroscopic viruses. In preliminary discussions certain problems and lines of action had been selected as suitable for the facilities which the Institute offered. An extension of the nucleus staff of pathologists was a recognized need of the new commitment, and the man to be sought was one with good experience in infective pathology, but especially one who had also the breadth of scientific discipline, the versatile ingenuity and the quickness of imagination, which might all be needed for advance into new territory by methods yet to be discovered. From experience gained during our intimate association in former years, I felt sure that Laidlaw had all these qualities in remarkable combination, and I had no difficulty in carrying that conviction with my colleagues who were more directly concerned in the matter. Laidlaw accepted the appointment and thus began the longest, last and most important period in his life as a research worker.

One of the diseases which had been selected for investigation was dog distemper. Carré had already produced evidence in support of the view that this extremely infective disease was due to a filterable virus. It was of great practical importance, and an effective method of prophylaxis or treatment would be enthusiastically welcomed by all who bred or kept dogs for sport or companionship. A factor in the choice, indeed, was the possibility of meeting a substantial part of the heavy expenditure which would be needed from a fund raised by *The Field* newspaper and offered in support of such an investigation. After a preliminary stage of organization, begun before he came to the Institute, the conduct of the research on this disease passed into Laidlaw's hands, with the co-operation on the veterinary side of G. W. Dunkin. The preliminary organization had been on an unusual scale, provision having to be made for the breeding of dogs through several generations under conditions securing their strict isolation, and that of their attendants, from the possibility of infection from outside contacts. It was thus possible to produce and

maintain a stock of experimental puppies, which were at the same time fully susceptible to distemper and free from the possibility of accidental infection. The late Captain S. R. Douglas, F.R.S., then director of the Department of Pathology at the Institute, and one of the most genial and unselfish of leaders, was quick to recognize Laidlaw's powers and to give him a free hand. Douglas was himself responsible for a suggestion, based on observations reported by game-keepers, which led to the successful trial of the ferret as an additional experimental animal, very susceptible to dog distemper; and arrangements had to be made for raising a stock of these animals also in complete isolation from infection. This introduction of the ferret, as a new subject for the experimental study of infections, later acquired an importance far beyond that which it had for the research on distemper.

While these plans for the experimental side of the work were completed and made effective, Laidlaw and Dunkin had begun their work with a detailed clinical and pathological study of true, uncomplicated distemper in dogs, to obtain a firm basis for its differentiation from a number of other infections to which the name 'distemper' had often been loosely applied for want of such clear definition. They passed the infection experimentally to dogs and ferrets, and gave accurate and complete descriptions of the uncomplicated disease in the two species. They were then able to produce conclusive evidence in support of Carré's earlier claim, that the primary infective agent, responsible for the transmission of this true dog distemper, is indeed a filterable virus. Then came the important problem of the nature of the very solid immunity to further infection acquired by an animal which had recovered from an attack, and the patient investigation, extending over some years, of methods of inducing this immunity artificially. Two methods were eventually found effective and practicable. In one the animal was first rendered partially immune by injections of the virus which had been inactivated by treatment with formaldehyde, and then received a small injection of the active virus, which caused an abortive attack and left the animal

with a strong and lasting immunity; in the other method, the animal received simultaneous injections of the active virus, and of a serum rich in the protective antibodies which Laidlaw and Dunkin had also prepared, the infection being thus checked at an early stage, and again leaving a permanent immunity. The fact that these methods, which they discovered and elaborated, have now passed into wide and successful use for the protection of young dogs from the disease which formerly killed so many, represents only one of the effects of Laidlaw and Dunkin's work. The systematic study of virus diseases was then in a relatively early stage, and they had to build on a correspondingly small foundation of existing knowledge. It would probably be difficult to point to any one item of their developed procedures as entirely original with them. On the other hand, it cannot be doubted that this early example of a complete and systematic investigation of a virus disease, and of the means of protection against it, by experimental methods which enabled exact and quantitative information to be obtained at every stage, made a landmark in research in this field, and profoundly influenced subsequent developments, which have now attained such imposing dimensions. The Royal Society recognized this in awarding a Royal Medal to Laidlaw in 1933, the President stating that 'both in its scientific and practical aspects this is one of the most successful investigations in medical science'.

Undoubtedly the experience gained in the work on dog distemper had the greatest importance for the other major advance in the knowledge of virus infections, with which Laidlaw was concerned. When the work on dog distemper was first planned, the choice had been influenced by a hope that a study of this disease, in which the experimental method could be freely used, might give a clue to methods of investigating certain important human infections. Among these, epidemic influenza had actually been mentioned because of certain analogies with distemper, both being highly infective, acute diseases primarily affecting the respiratory system; though there was, in fact, at that time no evidence to justify even a supposition that influenza was similarly

due to a filterable virus. The opportunity of putting this possibility to the test of experiment was presented in the early months of 1933 by the first well-defined outbreak of typical, epidemic influenza in this country since the great pandemic of 1918–19. Laidlaw, in collaboration with C. H. Andrewes and Wilson Smith, made a large number of attempts to transmit the infection, by filtrates of secretions from patients suffering from characteristic attacks of influenza, to a range of the animal species available for such experiments. The work on distemper was then only approaching completion, and Laidlaw's stock of healthy ferrets, kept in careful isolation from accidental infection, was still being maintained. It was this unique opportunity, aided by Laidlaw's now long experience in studying a virus infection in such animals, which enabled the team to discover a virus characteristic of the influenza epidemic; for the ferret alone, of all the animal species subjected to test, was found to be susceptible to the virus as directly obtained from the human patient. When this virus had been maintained by a number of successive transfers from ferret to ferret, it was later found possible by a suitable technique to transmit it also to mice. Later again, by an immediately unpleasant but scientifically fruitful accident, a co-worker of the team became infected from one of the experimental ferrets, and had a sharp and typical attack of influenza at a time when direct infection from human cases could be excluded.

This discovery of a virus as the infective agent of human epidemic influenza was quickly and abundantly confirmed by workers in a number of different parts of the world, wherever a suitable stock of ferrets could be made available in relation to an outbreak of epidemic influenza. Interesting collateral support was obtained from the work of R. E. Shope, in America, on a type of epizootic influenza occurring in that country in herds of swine; the virus from this swine disease was found to be similarly transmissible to the ferret, and to have characters closely similar to, though not identical with, those of the virus of human influenza.

In the years that have followed the primary observations

have been often repeated, extended and to some degree complicated by those of a large number of workers in different countries. With these further developments Laidlaw was less directly concerned, being content, when the main lines had been traced and verified, to leave the further elaboration to the colleagues who had shared with him the initiating discovery; though his experience, insight and resourcefulness remained always freely at their disposal for advice and consultation. It was certain, in any case, that research workers all over the world would soon be sharing, as they have done, in the exploitation of a find of this magnitude and potential importance. Progress towards a method of prophylaxis, such as Laidlaw and Dunkin's work had provided for distemper, has been delayed by the discovery that influenza in different epidemics is due, not to one uniform virus, but to a number of closely related but distinct strains; there may even be some which cannot yet be detected and maintained by transmission to lower animals. In these circumstances it is impossible to foresee what further period of progress will be necessary before an effective method of immunization against influenza becomes a practical possibility. It may be long postponed, or some unexpected gain of knowledge or method may make it suddenly available. Soon or late, there can be little doubt that it will come, and it is sad to think that Laidlaw will not be there to see the harvest of practical benefit reaped. We may hope that, when it comes, his part in discovering and sowing the seed will not be forgotten, and that his memory will receive an acknowledgment which he would have been the last to claim for himself.

Laidlaw's work on virus diseases, though it had the first place in his thoughts, by no means represented the whole of his scientific activities during these last eighteen years. When the work on dog distemper was already absorbing much of his attention, he still found time and freedom of thought for collaboration with other colleagues in entirely different fields of research. Such was his work with the late H. W. Dudley on a specific precipitating substance—a complex, carbohydrate haptene—from the tubercle bacillus, in which his persistent biochemical

interests found renewed scope. A longer and more productive collaboration was that with C. C. Dobell, on the cultivation of the dysentery amoeba and other entozoic amoebae in an artificial medium. Though cultures could be obtained by methods already then available, these were never rich or long lived. Dobell and Laidlaw recognized that carbohydrate was lacking in the system, but that, if sugar was added, the medium was rapidly rendered unsuitable by acid-producing bacteria. The way out of the difficulty, very characteristic again of Laidlaw's biochemical instinct and ingenuity, was found by supplying the carbohydrate as sterilized grains of solid rice starch, which the amoebae could freely ingest, and by eliminating those bacteria which can attack starch in this form. Under these conditions the parasitic amoebae grew profusely, and could be maintained indefinitely by sub-cultivation. Dobell and Laidlaw were able to investigate the action on such cultures of the alkaloids of ipecacuanha, and thus to give a new and accurate conception of the curative action of emetine in amoebic dysentery; while Dobell has used the method continuously since its discovery, in investigating the complete life cycles of the different species of amoebae found in the intestines of monkeys and of man. In more recent years Laidlaw invoked the collaboration of W. J. Elford in determining the physical characters and dimensions of a group of free-living but extremely minute organisms, which he had discovered in sewage. These grew readily in artificial culture, but passed through filters as readily as many viruses.

Apart from such varied collaborations, there were problems of his own on which Laidlaw worked steadily, undaunted by repeated failures, through many years. He nearly always had in hand, for example, some new attempt to devise an artificial medium which would allow a true virus to multiply in the absence of living cells; and for years he found time, at intervals, for renewed attack on the problem, which had baffled so many, of finding a method for cultivating the bacillus of human leprosy, or of transmitting it to some lower animal. At his suggestion Balfour Jones tested the susceptibility of the Syrian hamster to

rat leprosy. When the rapid spread of the rat disease in this species was thus demonstrated, it was Laidlaw, again, who encouraged Saul Adler, using his opportunity at Jerusalem, to test the possibility of transmitting human leprosy also to this rodent; and the apparent success of this trial may have far-reaching results for the further study of this terrible affliction and its treatment. One of the last of Laidlaw's own investigations was newly suggestive of a possible relation between the leprosy of rats and the human disease; but these results will not now receive publication, beyond the brief account which he gave of them for mention in the Medical Research Council's Annual Report for 1938–1939.

Apart from a less formal address to the Physical Society of Guy's Hospital, Laidlaw appears to have given only two lectures which came to publication, though these were of high standing—the Linacre Lecture on 'Experimental Influenza' and the Rede Lecture on 'Virus diseases and viruses', both at Cambridge. Laidlaw presented observations and ideas of permanent interest and value in both these lectures, but each of them involved him in a disproportionate agony of composition and anticipation. His contributions, indeed, to the advancement of knowledge in the field of the medical sciences are by no means fully represented by the published records which bear his name. Few men have given so freely of their knowledge and their ideas to assist the researches of others, and still fewer have been able to do so with effect over such a remarkable range of scientific problems. In the circle of those who were privileged to have daily contact with him at the National Institute for Medical Research, with our wide diversity of research activities, it was commonly accepted that Laidlaw was the one among us to whom any of the others could go with confidence, for helpful discussion, constructive criticism and frequently brilliant suggestion. The very qualities, however, which endeared him to a comparatively small and intimate circle of colleagues, must have made it difficult for others, and especially for men of science in other countries, to understand the full meaning of Laidlaw's work, and his in-

fluence on the progress of medical research in recent years. Even at the informal meetings of Societies, which he attended with growing difficulty and reluctance, he seldom made a communication or intervened in open discussion. It was in free and private talk that he was at his best, and drew most readily from the wealth of his knowledge and experience. Rather to his dismay, his knowledge, judgment and resourcefulness became, in recent years, more and more in demand on Committees, and on those dealing with veterinary problems as much as on those concerned with human medicine. Among other subjects to which he thus made a steady and solid contribution over many years, by inspiring and guiding the work of others, without publishing anything under his own name, was the pathology of dental decay.

On the death of S. R. Douglas, in 1936, Laidlaw succeeded him as Deputy-Director of the National Institute for Medical Research and head of the Department of Pathology and Bacteriology. He had never liked responsibility, but he was loyally ready to undertake irksome details of administration in the interests of the work of his colleagues, provided that he could do so unobtrusively and without open acknowledgment. To the detailed planning of the Pathological Section of the new Institute, now being built, and of a large section of its common facilities, he had devoted much time, effective thought and careful enquiry. If he had known that he would not live to share the results of his planning, I am sure that the knowledge would have made no difference to his effort. He had, indeed, made up his mind to retire, in any case, when he reached the age of sixty years. His joy in assisting the advance of knowledge and the keenness of his scientific instinct were undiminished to the end, but, as the years passed, his physical disability began to bear ever more hardly upon him. Golf and fishing, which had given him so much enjoyment, became impossible, travel fatigued him, and, as he was no longer fit for the kind of holiday which in earlier years had so renewed his energy and his sense of efficiency, it became difficult to persuade him to take any holiday at all. Death, which came quietly in his sleep, released him from a growing

discouragement at the steady decline of his resistance to fatigue; but it came with a shock of sudden bereavement to the colleagues who had come to depend upon him for so much. Only they knew fully what depth and precision of knowledge, what eager interest and inexhaustible ingenuity, were always at their disposal behind the light screen of his shyness and his economy of speech. And among those who had come to know his rare ability and the worth of his character, even from less frequent contacts, there were surely none whose affectionate regard was not awakened by this gentle, modest, generous man of science, and most loyal of friends and colleagues.

Laidlaw was elected to the Fellowship of the Royal Society in 1927, received a Royal Medal in 1933, served on the Council from 1934–1936, and then for a term on the Physiology Committee. He received the honour of knighthood in 1935. On the afternoon before he died he had shown me, with evident pleasure, a letter informing him that he had been made an Honorary Fellow of St John's College, Cambridge. He was unmarried, and, of the large family of which he was a member, only his mother, his sister and one brother have survived him.

H. H. Dale

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